

Bowditch (H. J.,)

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DR. BOWDITCH'S REJOINDER TO DR. PAINES "REPLY."

To the Editor of the Boston Medical and Surgical Journal.

SIR,—Will you allow the few remarks that follow to have a place in a Supplement to your Journal. I believe that Dr. Paine in his reply to me (Boston Med. and Surg. Journal, Vol. 23, page 183, &c.) has made many rash assertions in regard to Louis, which I think should not pass unnoticed. Still I am unwilling to ask for a place in the body of the Journal, as the subject has already occupied so much space. In the letter from M. Louis to me, it will be seen that he rather misinterpreted the precise object of my questions to him, and probably thought that Dr. P. had attempted to take from himself (M. Louis) the credit of the works under his name. I am not sorry, however, for the mistake, inasmuch as his answers incidentally bearing upon the points at issue between me and Dr. Paine, will be of more weight than if prepared for the special purpose of meeting my wants.

I do not think it necessary to enter into much detail in my rejoinder. The chief items which I stated in my first communication I believe still to be true, and notwithstanding Dr. Paine considers that, "actuated by the lowest motives," I have "thrown suspicions over the honest guardians of the press," and himself in particular, when "*it is apparent that he has had at heart the well being, alone, of his fellow men,*" I still do not retract. Moreover, having been accused of being too severely personal,* it gratifies me to find that one journal in this country sustains me,† and the able editor of the British and Foreign Medical Review,‡ as will be seen in the course of the succeeding pages, is by no means backward in bestowing a stinging rebuke upon Dr. Paine. The "forbearance" of the Medical Press, which Dr. Paine speaks of in his 5th number, is caused, I fear, by the same motives that induced a reviewer in one of the capitals of Great Britain, to say in a private letter to a friend, that he threw the book aside as one which "would carry us back to the dreamy theories" of the past, which "utterly despises the Baconian Philosophy," and finally, which is so diffuse, that one "half of it would never be read, and the other half would not be believed." Perhaps, as I suggested in my preface to the pamphlet of "Remarks," it would have been wiser to have suffered the whole to have fallen stillborn from the press, an acephalous fetus.

"Informe, ingens, cui lumen ademptum."—ÆN. 3.

But I felt called upon to repel so rude an assault, and as Dr. P. has seen fit to reply, I will not yield without at least pointing out some of the puerilities which he has blazoned forth in his "Reply." I say puerilities,

* Dunglison's Journal.

† Baltimore Med. and Surg. Journal.

‡ British and Foreign Med. Rev., April, 1841.

because no wise man would have displayed his own ignorance of the most common peculiarities of Louis's method. But you may rest assured, Mr. Editor, that if Dr. P. hereafter says anything upon this subject, or any other, I shall not trouble you or him with any criticisms upon it.

Mean while, as Dr. Paine has seen fit to honor me with a Latin quotation,* which I presume he thinks peculiarly applicable to myself, it is but right that I should be equally complimentary, and I therefore submit the following, from a writer much more ancient.

Very truly yours, H. I. B.

Πᾶσι ἡπίστατο ἔργα, καὶ ὡς δ' ἡπίστατο πάντα.—Μαργίτης.

Martin Paine, M.D., A.M.

SIR,—In your "Reply" to some strictures by H. I. B. upon your Commentaries, you say, in application to myself, that you regret to be obliged "to touch upon the morals of another" (183 Journ., 3 Pampl.);† whilst previously, without any personal acquaintance with Louis, you saw fit to accuse him of gross acts of injustice to his contemporaries. Your apparent dislike to treat me in the same manner seems to bear somewhat the impress of insincerity. However, whatever regards myself, I shall not notice; but how have you refuted my assertion that you had done injustice to Louis? By the following remark. "Shall we, for instance, pronounce the politician a 'liar' or 'a base private character,' because he disregards the public weal to carry out the objects of party, or even advance the aim of selfish ambition?" "It is exactly in the foregoing respect that I have considered M. Louis—publicly wrong, but probably amiable in private life." Your powers of argument must be more forcible than they are, Sir, before you can persuade me, and (I am disposed to think) many others, that any one guilty of gross public immorality can be an *amiable* citizen. Such a man is tainted with untruth in one act of his life, and the next moment becomes pure as driven snow! No, Sir, such an anomaly cannot exist, notwithstanding your assertion to the contrary.

You are quite anxious that every "loop-hole of escape" (189 Journal, 5 Pampl.) from utter annihilation may be closed against me. You seem quite certain of destroying my own reputation for justice, at the same time that you undermine my arguments, and therefore you inform us, in glaring capitals, whom you suppose the mysterious letters H. I. B. to indicate. My dear Sir, you take unusual pains to do what, had I not supposed that the readers of the Journal would have smiled at my *bonhomme* for doing, I should have done myself. I had a right to assail you anonymously; but as I knew I should be severe, I *scorned* to do so. The question then arose whether I should sign my name, and, as I suggest above, I thought that by so doing I should seem to be too much an imitator of other "commentators," i. e. I should put my name to remarks of very trifling importance to the public at large. I decided therefore to use my initials, as these would be sufficient for *yourself* and for any of our friends who should take any interest in our discussion. But as you have seen fit to publish the name, the burden of the folly rests upon yourself.

* Male cuncta ministrat impetus.—Proverb.

† I shall give the pages of the Journal and of Dr. Paine's pamphlet.

You accuse me (188 Journ. 5 Pampl.) of condemning my own translation. The unjust character of this assertion will appear plain when I state that you have quoted more than two hundred passages from my translation of Louis's works, and in one instance even preferred mine to Dr. Cowan's. Yet in two instances alone have I referred to slight inaccuracies, from inattention to which, however, you have deduced important results. In one case a semicolon was placed where a comma should have been. You read as far as the semicolon, and then indited a column of "commentaries;" but an accurate reader would have at least perused the paragraph. The other error was equally insignificant. But without further argument upon this point, I stand at issue upon the general idea of any one becoming a "philosophical" commentator upon mere translations. You say, "suppose Muller, for instance, whom I have quoted largely and critically, should start the same objection." Well, Sir, in my opinion he would have as much right to complain of you for using Dr. Baly's translation, as Dr. Carpenter has for your criticism of his doctrines merely upon the knowledge you have gained of *his opinions from a review!* We usually have believed that the best translation rarely conveys to a *ripe scholar* the spirit and truth of the original; but it seems that in the progress of *Philosophical Commentaries*, we may now neglect originals altogether. Can we judge of Homer from Pope? or of the noble tragedies of Sophocles from the dull Francklin? Do we judge of Cicero's philosophical writings from translations? How, then, Sir, can you pretend to be so fond of some of the highest forms of criticism, and yet do not go to the originals of the works you speak of, trusting now to a translation which may have been made by a mere tyro for the sake of a little ephemeral applause, and again to the pages of a general review! It seems to me that upon your own admission your volumes are very faulty in their ground work.

I shall here introduce the following correspondence, which has *some* bearing upon the topics afterwards discussed in your reply.

COPY OF A LETTER TO MONS. LOUIS.

Boston, 28th Dec. 1840.

MY DEAR SIR,—I have sent you some copies of a small pamphlet which I printed lately in reference to a large work published by Dr. Paine, of New York. I hope that you have received them. Dr. Paine has answered me, and now I am desirous of obtaining some replies from you to the following questions.

1. In what relation did you stand to Mr. Chomel during the time that you were engaged in collecting the observations concerning which you speak in your advertisement to the Treatise on the Typhoid Affection?
2. Were you the chief physician of those patients whose histories you have there given us?
3. Were the prescriptions made by yourself or M. Chomel?
4. Do you mean to say that you had the sole treatment of the persons whose histories are given in the above work?

5. Did M. Chomel delegate to your charge a certain number of patients in order that you might experiment upon them, and if so, were any among those mentioned as above committed to you?

6. Is it or is it not true that the seven hundred cases mentioned in your advertisement (of diseases differing from the typhoid affection) actually came under your own observation?

7. Was it your intention that these seven hundred cases should form the basis of your work, as much as the one hundred and thirty eight cases of the typhoid affection?

8. What species of analysis did you make of these seven hundred cases?

9. What is the meaning of the word "Recueillir?" that is to say, what do you mean by the expression "J'ai recueilli de 1822 a 1827, &c." page 9, Advertisement?

10. Were there other records than your own of the symptoms, which you could have examined in making your analysis?

11. When you were in the service of M. Chomel were there other records made for the hospital archives? If so, please describe them, and if you made use of them, in what manner did you use them?

12. If you made use of any other records than your own, will you explain to me to what extent you did so?

13. How many apartments were entrusted to the care of M. Chomel, —what were their names, and how many patients were usually under his treatment?

14. Were the apartments St. Joseph and St. Jean confided to you?

If you will have the kindness to answer, as soon as possible, by the address mentioned below, these questions, I shall feel very much gratified, for although I have very little doubt with reference to the true answer to each question, I prefer to be entirely sure.

Accept, my dear Sir, my sentiments of esteem.

H. I. BOWDITCH.

MONS. LOUIS TO DR. BOWDITCH.

Feb. 8, 1841.

I have received, my dear friend, the pamphlet which you published in reference to Dr. Paine's work, and I hope likewise that before this you have received the second edition of my researches upon the typhoid affection. As I have not seen Dr. Paine's work, I can scarce conceive of the importance which you attach to the answers which you desire to the questions that you propose. Nevertheless, I hasten to give them, and send them according to the address which you have mentioned.

1. I stood in regard to him merely in the light of friendship, and I observed in the apartments *St. Jean and St. Joseph* which were entrusted to him. The only advice I had from M. Chomel was, to give more extent to my researches upon Phthisis, to modify a little the order which I had adopted in the arrangement of the objects of interest in the study of the Typhoid Affection. M. Chomel never gave me any further counsel, and it is my work alone,—a work, too, that caused him to abandon his previous views of fever. If Mr. Paine thinks that I was dependent upon

him for any other assistance than the privilege of observing in his apartments, he is wholly deceived. I am under no scientific obligations to M. Chomel, while, on the other hand, my researches at the hospital La Charité may have been very useful to him.

2. I was in no respect the physician of the patients whose histories I collected. I observed the condition of the patients, and nothing more; in the same way that I did at Gibraltar when examining the Yellow Fever. At the latter place I was frequently requested to undertake to treat the patients, and as constantly refused, because I wished to remain entirely in my station of simple observer. It is evident, in fact, that we must have more confidence in the results drawn from patients whom we simply observe, without at the same time treating them, than we should from similar results from patients whom we are observing and treating at the same time. Before my second edition of the work on Fever, I never deduced any inferences on Therapeutics, save from the observations of others, and it is only in this second edition, together with that of my researches upon Blood-letting and Tartarized Antimony, that I have given the results of my practice.

3. Every prescription was made by M. Chomel.

4. No one of the patients whose cases I have given in my researches upon the typhoid affection, were submitted to my treatment.

5. M. Chomel never gave me any patients to treat, and had he wished to do so, I should have refused, for the reasons mentioned above.

6. It is evident from the expressions made use of in the first edition of my researches upon the typhoid affection, that the histories of the patients of which I have given an analysis, were collected by *me*, and that I saw and studied all those patients.

7. My researches upon the typhoid affection rest, at the same time, upon the 188 cases of this affection which I analyzed, and upon the 700 observations of other acute diseases which I likewise analyzed.

8. The analysis which I made of these last (700) cases, consisted in comparing, by means of the numerical method, the symptoms and lesions found in the individuals affected with the typhoid affection, with the symptoms and lesions found in individuals affected with other acute diseases. This is evident from the perusal of the smallest portion of my work, as well as from its entire contents.

9. When I say "*J'ai recueilli l'histoire*" of a patient, I mean to express that I noted, day by day, in my notes, every circumstance that the patient has presented to my observation. [*"Quand je dis que j'ai recueilli l'histoire d'un malade, je veux exprimer que j'ai consigné, jour par jour, dans mes notes, tout ce que le malade offrait à mon observation."* I beg the reader to remember that out of my translation of "*recueillir*" Dr. P. makes a great plot to deceive my medical brethren.]

10, 11, 12. There is no record of symptoms kept in the hospitals in Paris, therefore I could not consult them; but if there had been millions, I should never have had recourse to them, for the reasons mentioned in the advertisement to my researches upon phthisis, my memoir upon the proper method of arriving at general facts, viz. because it is difficult to observe, and because when I began to pursue my researches no one

had such confidence in such records as would have led me to place entire faith in any that might have been kept at that epoch. Nothing but prejudice (*mauvaise foi*), as it seems to me, will deny that my researches have had an influence upon the exact method of observation which prevails now, and that no one before me had pointed out the difficulty of observation—a new proof that, generally, observations were made with very little exactness.

In regard to numerical analysis, I would remark that although some may have made attempts before I tried it, it was by instinct, and in this manner solely; but no one ever made a law of it, whereby to arrive at the knowledge of truth. The numerical method has been used by physicians only since yesterday, as it were; it is a wholly new thing, and all, or nearly all, the works by physicians prove it. M. Chomel has hardly done me complete justice upon this point. Mess. Marc D'Espine, of Geneva, Valleix, and finally Prof. Forget of Strasburg, are the only persons who have attributed to me what really belongs to me. (See the last page of the Treatise of M. Forget upon *Enterite Folliculeuse*.)

I advise you, my dear friend, in furtherance of the object that you have in view, to read the preface to the second edition of my researches upon the typhoid affection, and that to the Examination of the Review of my works by M. Broussais. You will have no difficulty afterwards, in demonstrating that if any one wishes to prove that I have been guided, he is mistaken, since my results destroyed M. Chomel's work upon Fevers, &c. How could I have been guided by a man who, although certainly of great talent, employed all the force of his mind and of his time in sustaining the doctrine of the essential nature of fevers? He evidently was not following a good method, and like his contemporaries did not seem to be at all aware that the imperfect condition of medicine depended mainly upon incomplete observation, and to a still more imperfect analysis of facts, I had almost said a total absence of all analysis. For what physician before this ever laid down for himself a law never to state a general proposition, unless it was the rigorous expression of the numerical analysis of a greater or less number of facts. If any general proposition has been stated without its being a rigorous expression of facts, I disavow it, and my friends should do the same, and not consider such a proposition except as the effect of a heedless mode of reasoning.

I hope, my dear friend, that this letter will arrive in season. I have with pleasure kept you in remembrance among your good friends at Paris. My wife thanks you for your recollection of her. We are all very well, and hope that you are so likewise. * * * * Do not forget to remember me to Mess. Jackson, Warren, Shattuck, &c., and receive, my dear friend, the renewed expression of my sincere attachment.

(Signed)

LOUIS.

I doubt not, Sir, that you will find enough to carp at in the above letter, and that it will serve, in your opinion, to sustain the idea set forth that Louis runs high over the ruins of others. But at present I wish to use the epistle for a different purpose from any discussion upon this subject.

Now, sir, the pages 189 to 193 Journal, or 7 to 10 Pamphlet, are

occupied in attempting to prove that Louis had the care of the patients, and was responsible for their treatment—and in answer to the whole of these pages I refer to Louis's response to my first four questions. By these answers your whole fabric is overthrown. I will therefore pass over them, with a single remark upon your closing lament at being obliged to use "any language that may be offensive to a chastened taste; but vulgarity may not always be rebuked by forbearance," &c. You perhaps forget that you accuse Louis of condemning to the *guillotine* (your own *Italics*) all his predecessors, most of his contemporaries, and threatening even posterity "with the same fate if it do not believe in him." Have you not said that he "fats himself upon other men's ruins"? that "he knows no road to fame, but over the ruins of others"? and have you not by what doubtless you supposed to be a very apt quotation, applied to him the very polite epithet of *confounded fool*? (your *Italics*.) How sadly you must feel to be obliged by my "vulgarity" towards you, to use language improper for chastened ears! Your remarks are so *polished* in reference to one who never reviled you, that no one for a moment can doubt that it must be very disagreeable to you to feel obliged to speak harshly of one who has rebuked you freely and frankly, but, at the same time, truly.

In your second number you prepare several articles of indictment, which certainly seem very severe; but in some manner they seem to elude my grasp, as I proceed: and several friends have noticed the same peculiarity. The assertions are made, but, contrary to what happens in courts of justice, we have no verdict returned. The articles disappear mainly in *tenuis auras*. In regard to what I stated about the number of cases used by Louis, I refer to my Remarks, as I do not conceive that your answer needs any criticism. But on page 204 J. (or 14 P.) you accuse me of altering my translation for the purpose of injuring you. You will doubtless scarcely believe me, when I say that the *plot* you discover on my part reminds me of the famous political plot which occurred in your city a short time previously to the recent presidential canvass. It existed alone in the brains of the conceivers of it; but, alas! the public could not be made to believe a word of it. The difference between my present and former translation arose in this way. Upon examining the *original* work (according to the rule so very offensive to you) I sought to give as accurate a translation as I could from my knowledge of Louis's usual method of observing. I did so without in the least thinking of my previous translation. My sole aim was to state the truth as clearly as I could. Yet you, sir, dare to say that my "new translation is false," and subsequently you accuse me of wilfully stating what I knew to be untrue, and that I knew my first translation was the more correct. Now I wish the reader to understand that this plot arose from my having translated at first the word "*recueillir*" by the very indefinite, so far as Louis is concerned, but correct in its general signification, word "collect." You, sir, evidently meant to insinuate that Louis had never carefully examined and recorded any but 133 cases; whereby one of the chief elements in the support of the Researches on typhoid would have been destroyed. But I knew he had done more, and therefore I said that he

"carefully recorded" (instead of "collected"), because I knew, from personal intercourse, that that was the signification in his mind of that term. But I refer you to 9th answer of his letter, and in that you will find that I have very exactly translated the whole. Where, now, is my plot, in the exposure of which you use nearly 10 pages of your pamphlet!

Again, in regard to your assertion that I endeavor to mislead my medical brethren about the 700 cases, also to his use of other records, and the foolish assumption that Louis had the wards St. John and St. Joseph committed to his care, I refer you to answers 5, 6, 7, 8, and 10, 11, 12, and 13; and by these I undermine your whole structure of infamy, described between pages 200 and 210 J., (12 and 20 P.) and which you hoped would weigh me down. It falls back upon the builder, and thus I leave this chapter and pass to your 3d article.

From page 216 to 220 J., (or 20 to 24 P.) you endeavor to prove that Louis ought not to be allowed anything which he does not grant to others. You say he should have given the histories of all his patients, so that we might judge of their merits. It seems to me that you require of him rather too much. The record of 58 cases merely of the typhoid disease swells the work upon that disease to twice the size that it would have were no observations given. Now if the 800 "carefully recorded" cases had been printed, we should have had an enormous number of volumes. I am willing to allow, if you wish it, that he ought to have printed them, and yet I would ask you whether any one would have had more faith in him for so doing. I do not mean that you should infer that we ought to have equal confidence in Dr. Perry's 4000 cases, as we have in the manuscript cases of Louis; for Louis has demonstrated in the cases he has detailed, his accuracy of observation and skill in diagnosis. Dr. Perry may be equally accurate, but we may have no proof.

But the most amusing portion of this article is that in which you assert that "you have unravelled the snare, and made out fully your specifications." Truly, as I have already hinted, I cannot see how you have proved all your articles of indictment, even if your own evidence had not been controverted by Louis's letter. I am perhaps somewhat blinded, but I was much struck with the truth of the following remark which I find in the British and Foreign Medical Review, April, 1841. The writer is criticizing your Commentaries, and the remark seems to apply very well to the present case: "But we fancy that his mind must be deficient in one qualification, which is rather important for one who sets up for a philosopher (a title of which we should judge by the heading of four of his essays that the writer is rather ambitious), namely, the capability of perceiving accurately the relation of ideas. This notion we have formed from the multitude of instances of loose and incoherent reasoning, of contradictory statements, and of misinterpretations of the opinions of others, that we have detected in his works." But perhaps this is *sufficient* for this article.

Article 4. You apply the epithet "*atrocious*" (page 233 J., or 25 P.) to my assertions that you "never let the reader lose sight of the main object of your work, viz. a violent attack upon the Numerical, or, as he chooses to call it, the Anatomical School." In calling Louis the Rival

Anatomist of Andral (Vol. 2, 558); in speaking of one of Louis's cases as one of the strong cases of the Anatomical School (Vol. 2, 631), in quoting Louis's remarks, and adding thereunto "or the Anatomical School" (Vol. 2, 641), I presumed at least that you thought these two schools to be nearly allied, identified in fact, or that the Numerical was swallowed up in the Anatomical. Now, as in my Remarks I proved that Louis, though valuing highly Morbid Anatomy, is continually advising the student to examine carefully the symptoms, I consider that the Numerical School stands upon higher ground than you allow. It endeavors to attend accurately to Physiology, to the Symptoms of Disease, and Morbid Anatomy. I regard Louis as the incarnation, if I may so say, of the Numerical Theory of Medicine, but by the above remarks you make him a mere anatomist. This is what I complain of. The strict follower of the Numerical Method stands far above the Morbid Anatomist. But you too frequently, as we have seen above, use the terms Numerical and Anatomical as equally applicable to the same person. In the technical sense of these words they should not be thus used.

Again, you complain that I state that you never let us "lose sight of the main object," viz. "a violent attack upon the numerical school." Perhaps "*a*" would have been more appropriate than *the*; nevertheless, I wish to enumerate the reasons why I made the remark, viz.: 1st, because of the long chapter devoted exclusively to the writings of Louis, which are the only *perfect* specimens of the Numerical Method in existence. If the results contained therein are useless, the method, so far as it was pursued in arriving at these results, is useless, so intimately are the two connected. In other words, the Numerical Method is attacked. It is useless for you to deny this, for Louis's writings rest wholly upon this method; it runs through all his works, and any one who attacks one as you have done, must necessarily attack the other. But, 2d, in addition to this chapter, I repeat that you never let us lose sight of your determination to make a violent attack upon Louis and his writings, which, according to these views, is synonymous with opposition to the Numerical Method.

You say, sir, these are misrepresentations, and that they are "atrocious." Let us go a little further. I have spoken of the long chapter devoted to Louis, and the reader will observe that to no one individual have you shown so much *honor*, in this respect, as to him; for to him and his writings alone have you devoted this whole chapter. But we have likewise constant reference to them through both volumes, and as you seem so much startled at this assertion, I have taken the liberty of examining again still more carefully this point, and I find it true, numerically, that in as many as forty different passages, in the two volumes, you allude to, and sometimes abuse most heroically, M. Louis, his works, or his method. Only seven times in this number do you quote him with pleasure, and usually on these occasions it is to help prove a point that you wish to gain. In some of the cases it is true that, like the mouse's head from behind the Acanthus leaf, your spirit of hostility *peeps out* cautiously, or in the form of a sneer. Nevertheless, it is always evident, to a cautious eye. To refresh your memory upon these points, I refer you to

the following pages : 230, 238, 282, 293 to 303 (violent), 305, 314, 331, 408, 533, 626, 681, 696, of Vol. 1 ; 195, 249, 250, 280, 302, 320, 327, 335, 431, 432, 451, 452, 503, 531, 534, 536, 542, 558, 574, 586, 617, 618, 626, 629, 631-2-3, 641, 645-6-7-8, of Vol. 2.

Now if these do not prove that the main object of your volumes was what I have stated it to be, at least they prove that amid almost all your labors, you scarcely ever forget your dislike of Louis and his Method ; and they certainly prove the truth of my assertion, that "there is scarcely a hundred successive pages in either volume in which this opposition does not manifest itself." So much for the first four pages of this article.

On page 236 J., or 28 P., you state that Louis neglects various considerations of climate, constitution, habits, age, sex, &c. Please, sir, read his essay on Clinical Instruction,* wherein you will find much stress laid upon the necessity of studying these very subjects.

On page 239 J., or 31 P., you have another plot, and in answer I would merely refer the reader to my Remarks, pages 13-14, where he will see that the alteration of the follicles, according to Chomel, is "a lesion wholly peculiar to the typhoid affection." But I will waste no time upon this point. I retract not an iota. The only way in which I can account for your assertion, is that from your dislike of every thing like an autopsy, you must be ignorant of the distinctions which have been made in pathological anatomy in modern times. I confidently refer the reader to Chomel's Lectures† in proof of my truth.

In Article 5 we have a renewal of the charge about the generalizations derived from the "debris of the body." It is useless to repeat anything upon this subject, but there is a good remark in the Article above referred to. In criticizing your essay upon the Schools, the writer thus expresses himself : "He seems to forget how much the observation of phenomena during life has been guided by the knowledge derived from post-mortem examinations, and how frequently the value of a particular sign would be lost if it were not connected in the mind of the practitioner with a coincident morbid state ascertained, by post-mortem examination of similar cases, to be probably existing. * * * It is an old observation, that we do not know the value of a blessing until we have lost it ; and we would say, in like manner, that we could scarcely tell how much of our knowledge of the phenomena of disease in the living state is really due to post-mortem anatomical researches, unless we were entirely deprived of the assistance we have derived from that source."‡

On page 271 J., and 34 P., are remarks from which we must infer that, according to you, not one case of all the patients that enter the numerous hospitals in Paris, is worthy of record ; for although you apply the remark to Louis's cases, you use arguments that will apply to all. I have really supposed that it was of some use to visit Paris for Medical Instruction. I have thought that something was to be learned from the Parisian Hospitals. But it appears that I have been in error. "Their (the patients') constitutions are wretched, their treatment worse, and they are full

* Pages 3 and 4, Martin's Translation, 1834.

† Lecon's Clinique Med. 1831.

‡ British and Foreign Medical Review, April, 1841, p. 401.

of organic lesions before they enter the Gallic Hospitals." Such bold remarks well become a philosophic spirit! Yet I would ask, have we received no light of science from these same hospitals of Paris? Where have all the great Frenchmen of ancient and modern days been laboring? But their labor profited them not, you say, for they are workers upon a sterile soil! Really, sir, this is a rash, not to say foolish remark. How many do you suppose you will find to agree with you? Doubtless in some respects we must modify our opinions when bringing the results of merely hospital practice to the test of every-day life; but to assert that nothing of importance can be derived from them, seems to me to be, at least, very extraordinary!

On pages 273-4 J., and 36-7 P., you attempt to impale me upon both horns of a dilemma; but I do not feel that you have succeeded, except in proving your own peculiar unskilfulness in drawing inferences.

Mons. Louis, if he insinuates anything, insinuates that diarrhœa *can* exist without appreciable lesion of structure. But you now say that you meant to state that Louis left the reader in doubt about his meaning. You were peculiarly unfortunate in your use of terms. And on page 275 J., or 37 P., you are so strangely dogmatical, as to assert that because Louis says that in nearly all the cases of typhoid disease the patches were the only part of the canal that was diseased, and consequently the only part in that case to which diarrhœa could be referred, therefore "this looks a good deal like denying that diarrhœa can arise from mere functional disease." How so? If after a certain symptom we find one part alone diseased, we may justly be led to refer the symptom to that part; but it by no means follows that we should deny the existence of functional disease from that fact alone. Is typhoid fever the sole disease that causes diarrhœa; and as we have stated, does not Louis, in the very quotation you have formerly presented, allow of simple functional disease?

I pass now to the part in which the abrupt termination of your publication in the Journal took place. I shall leave the rest of the pamphlet to speak for itself. You in that part accuse me of dealing unjustly by you,—of attacking your reputation as a man and writer. Sir, when I undertook the refutation of your attack upon M. Louis, I knew not who Martyn Paine was. I had not the advantage the English critic had, of seeing your name under the title of Professor, which appears upon the *English* advertisements of your work. I knew you merely as a man who had, I thought, dealt unjustly with my friend and instructor. I freely confess that from the same motives that I would defend the reputation of a father from the attacks of a foe, I endeavored to repel yours upon Louis. You complain of my warmth. Pray, sir, did you not commence the combat by abusive epithets upon another? I quoted specimens of the coarse language with which you attacked a man alike honored and beloved by me, for in addition to being to me an able instructor, he watched over me with parental tenderness while alone and ill in a foreign land. Be assured that nothing but a sense of almost filial duty impelled me to the task which I have undertaken. I am perfectly willing to trust my reputation to posterity, and I believe it will not be less honorable in con-

sequence of these communications with you. I must confess, however, that I do not hope for much good or evil report with the future, notwithstanding my name is to be associated with your own in what you may suppose an unenviable proximity; for I fear that the dust of ages will settle upon us both, unless we do more than either yet has done.

I remain,

HENRY I. BOWDITCH.